Dear Editor and Reviewers,

We sincerely thank you for the time, effort, and expertise you have dedicated to evaluating our manuscript. We deeply appreciate your constructive comments and insightful suggestions. Incorporating your feedback has allowed us to refine our data presentation and clarify our methodological approach, resulting in a substantially stronger and more robust version of this paper.

Please find our detailed, point-by-point responses to each of the reviewers' comments below.

Reviewer 1

L 44 : GHG abbreviation not defined

We have defined the acronym in the first paragraph

L 47: “CO equivalent” ?

This has been corrected in the text, changing CO by CO2

L 55: you could consider also adding the synthesis by Rosentreter et al. (2021)

The reference of Rosentreter et al 2021 has been included, thanks for the suggestion

L 56: “Similarly, N2O accounts for about one-third of the natural sources to the atmosphere” there seems that something missing from this sentence. “natural sources” of what ?

Thanks for pointing out this mistake. Sentence now says: “Similarly, the ocean accounts for about one-third of the natural N_2O sources to the atmosphere”

L58: also DIN

Thanks, we included DIN.

L 61: “extensive coastline” but very narrow continental shelf, so that most of the Mediterranean is deep ocean. Major truly coastal regions in the Mediterranean (Adriatic Sea and Aegean Sea) were not sampled.

We thank the reviewer for this valid observation. While the original sentence was intended as a broad geographic introduction, we agree that emphasizing the narrow nature of the continental shelf is more accurate and relevant for coastal biogeochemistry. We have modified the text in the Introduction to reflect this distinction.

L67: is warming rate equivalent in the Eastern and Western Mediterranean regions ?

No, the warming rates are not equivalent. Sea surface temperature warms faster in the Eastern Mediterranean (~0.048 °C/year) than in the Western Mediterranean (~0.036 °C/year). Conversely, the upper water column (5–700 m) warms faster in the Western basin (~0.070 °C/year). We have clarified this regional and depth-dependent distinction in the revised text.

L76-85: there are some extra measurements of CH4 in seagrass meadows (Champenois et al. 2021).

We have included this reference in the text as a comment “and minor CH4 source from seagrass meadow reported in Corsica (Champenois et al., 2021)

L 87-89: I do not think it is “vital” to expand data collection for CH4 in the ocean. The ocean has been repeatedly shown to be a very minor natural source of CH4 to the atmosphere, and mainly confined to shallow nearshore regions. It does not mean it is not interesting to measure CH4 in the ocean, but the choice of words is important, and “vital” seem inadequate here.

We agree with the reviewer's assessment and have replaced 'vital' with 'valuable' to more accurately reflect the context and magnitude of marine CH4 emissions

L109: As far as I’m aware reporting GHG marine emissions is not an obligation of the Marine Strategy Framework Directive. But I can be mistaken, please provide more details (reference or link).

The reviewer is correct. Reporting marine greenhouse gas emissions is not a mandated obligation under the Marine Strategy Framework Directive (Directive 2008/56/EC). We have removed the inaccurate statement from the text."

L321: Scripps’ CRMs are used to calibrate total alkalinity, pH, and DIC measurements. However, CH4 and N2O are methodologically closer to pCO2 measurements which are not calibrated with Scripps’ CRMs. Instead, certified calibration gas cylinders are used to calibrate pCO2 measurements, for instance from NOAA. I think that there are equivalent certified gas cylinders for CH4 and N2O measurements that can be used to calibrate the GCs.

"CRMs serve as aqueous-phase reference materials, matching the dissolved nature of our samples. NOAA standards are used strictly to calibrate the gas analyzer for CO2, N2O, and CH4. As detailed in line 314, we utilize NOAA standards. In addition, we used two provided by SCOR WG 143 (see Wilson et al., 2018)—as primary standards to improve the accuracy our Air-Liquide working standards, which have a higher production uncertainty (5-10%).

However, the intention of this specific paragraph is to discuss the overall quality standard for dissolved gas analysis in the aqueous seawater sample. This encompasses both the gas-phase measurement and the entire headspace equilibration process required to extract the gas from the dissolved phase. To clarify this distinction and avoid confusion, we have added the phrase 'in the aqueous phase' to the revised text."

L 325: please provide detection and quantification limits of CH4 and N2O measurements.

We have computed the detection and quantification limits and included a paragraph in the method section “Instrumental limits of detection (LOD) and quantification (LOQ) for the Agilent 7890-GC were defined as 3 and 10 times the standard deviation of triplicate injections of the lowest standard (NOAA atmospheric level), divided by the calibration slope, respectively. This yielded LOD ranges of 3–10 ppbv for CH_4 and 0.1–2.9 ppbv for N_2O, and LOQ ranges of 9.8–32.4 ppbv for CH_4 and 0.3–9.7 ppbv for N_2O. Because dissolved gas analysis is fundamentally constrained by detector sensitivity, conservative methodological limits were calculated by substituting the maximum gas-phase LOD and LOQ values into Eq. 1 for seawater at S = 35

and T = 20 °C. The resulting aqueous LODs were 0.11 nmol L^{-1} (CH_4) and 0.16 nmol L^{-1} (N_2O), with corresponding LOQs of 0.35 nmol L^{-1} (CH_4) and 0.55 nmol L^{-1} (N_2O)".

L 329: section 2.3: there is a need to explain how the Ceq was derived (from which atmospheric pCH4 and pN2O values).

We thank the reviewer for this observation. The calculation of Ceq, including the specific atmospheric pCH4 and pN2O values used, is explicitly detailed in lines 345. To avoid redundancy while ensuring clarity, we have added the phrase "as previously defined" at line 364 to direct the reader to the original derivation.

"...where k is the gas transfer velocity (cm h^{-1}) and ($C_{meas} - C_{equi}$) is the air-sea concentration gradient of N_2O and CH_4, being C_{meas} and C_{equil} the measured and equilibrium gas concentrations defined in the previous section."

Table 2: "Fosfate" ?

Thanks for your comments, we have corrected the typographical error, replacing "fosfate" with "phosphate" throughout the revised manuscript.

Figure 2 = there are 2 CH4 values for LEV-B1 (?) that seem to be = 0. Is this real of data that were not filtered correctly ?Figure 2 = the figure is difficult to read because round filled symbols are used for all the stations. It would be advisable to use a combination of different symbols (circles, squares, triangles, diamonds) filled/open to improve readability.

We thank the reviewer for noting this. We have corrected the figure; the samples in question were damaged during transit, and the software erroneously plotted the resulting null values as zeros. In addition, we have changed the symbols in figure 2 and 4, to improve readability.

L488 : section 3.3.: it is strange that CH_4 was not tested against station depth, as there is ample evidence that CH4 strongly correlates with depth in the ocean (Weber et al.).

We appreciate the reviewer's point regarding the depth dependence of CH4 (Weber et al., 2019). We initially included station depth in our correlation matrix but found no significant relationship. Furthermore, we tested for correlations among stations within the eastern and western basins separately, which also yielded no significant depth-dependent variability. This indicates that the CH4 variability found among our stations does not correspond to depth-dependent benthic remineralization, but is instead driven by localized sedimentary sources. We have explicitly clarified this in Section 3.3 with the text:

" Although oceanic CH_4 often exhibits a strong depth dependence (Weber et al., 2019), station depth was included in our initial correlation matrix but yielded no significant relationship ($p > 0.05$). This lack of correlation indicates that CH_4 variability found among stations in our study in our study area is primarily driven by other sedimentary sources rather than depth-dependent benthic remineralization."

It is notable that there is no correlation between CH_4 and PO_4^{3-} (p=0.01). Indeed, there has been emphatic claims about aerobic CH_4 production in PO_4^{3-} depleted areas of the ocean

(Wang et al. 2026), but this does not seem to apply in the Mediterranean Sea despite the fact that this ocean region is an oligotrophic zone.

We agree with the reviewer that aerobic CH4 production linked to phosphate depletion is a key pathway in open-ocean oligotrophic regimes, such as the HOT station (Wilson et al., 2017). However, coastal dynamics fundamentally differ from open-ocean systems. In our coastal stations, CH4 variability and is strongly dominated by seafloor emissions. These significant sedimentary inputs result in much higher background CH4 concentrations that effectively mask any subtle any possible *in situ* aerobic production signal. Consequently, the methane and phosphate coupling observed in stable open-ocean time series is neither expected nor detectable in our heterogeneous coastal dataset.

Wilson, S. T., Ferrón, S., & Karl, D. M. (2017). Interannual variability of methane and nitrous oxide in the North Pacific Subtropical Gyre. Geophysical Research Letters, 44, 9885–9892. https://doi.org/10.1002/2017GL074458

Reviewer 2

This paper presents a valuable and comprehensive dataset of surface N_2O and CH_4 concentrations and air-sea fluxes collected in the coastal regions of the Mediterranean Sea. I would say this study was designed well and presented clearly. I think the data can enhance our understanding of variation in non-CO_2 greenhouse gases concentrations and their influencing factors in this region. The topic fits within ESSD's scope. Thus, I recommend publication only after minor revisions. The following comments are for the author's reference.

We sincerely thank you for the time, effort, and expertise you have dedicated to evaluating our manuscript.

1. Line 325: 'samples were discarded with a CV higher than 4.5% (n=3) for CH_4 and1% for N_2O (n=1)'. The removal procedure of outlier datapoints for N_2O needs justification. I am wondering if it may remove real "hotspot" observations rather than erroneous measurements.

   The reviewer's concern is understandable, but the stated coefficient of variation (CV) thresholds apply exclusively to duplicates taken from the same sample. This quality control step is not related with manipulation and storage of samples —such as handling contamination or leaks—rather than genuine environmental hotspots. We have amended the text to explicitly state that these CVs represent analytical precision between duplicates

2. Line 492: 'there was a positive correlation between N_2O_{Sat} and temperature', please give the fit equation, n, r and p value.

   We thank the reviewer for the suggestion. As requested, we have added the linear fit equation, the sample size (n), the correlation coefficient (r), and the p-value for the relationship between N2OSat and temperature to Section 3.3. ($N_2OSat = 0.82*T+89.96$; $r=0.58$, $n=109$, $p=4.4*10^{-11}$)

3. Figures 2 and 4: It's better to use different symbols together with colors to distinguish the data from different study sites more clearly.

We thank the reviewers for noting this, we have changed the symbols in figure 2 and 4, to improve readability.

4. Figure 3: The figure captions should give more detailed information, for example, what does x represent? Which represents mean and median values?

   We thank the reviewer for the observation. We have updated the caption of Figure 3 to explicitly define the x-axis (acronyms for sampling stations) and clarify which symbols represent the mean, median, quartiles, and whiskers. The listed acronyms correspond to: Ras El Ma= REM; M'diq=MDIQ, Palma Bay =PB; Cabrera =CA; Cape Salines= CS; Erdemli Time Series =ETS; Beirut=BE; Batroun =B1

5. Tables: I recommend using a three-line table.

   Thanks for the comment, we have modified all the tables in the manuscript

6. Table 4: please provide the CH_4 mean value for Strait of Gibraltar

   We thank the reviewer for the observation. Table 4 presents the range of observed CH4 values rather than a mean for the Strait of Gibraltar. Given our incomplete seasonal coverage in this specific region, calculating a mean would introduce temporal bias. We opted to report the observed range to accurately reflect the measured concentrations without misrepresenting the annual baseline and include a note in the head of the table reporting this."